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REVIEW ARTICLE
A NEW SYNTHESIS OF EARLY MEDIEVAL BRITTONIC HISTORY

Wales and the Britons 350–1064. Thomas M. Charles-Edwards. Oxford University Press, Oxford, 2013. xx + 795 pp. £102.50 hardback, £30 paperback. ISBN 978-0-19-821731-2.

WE have been waiting a very long time for this book. Though billed as ‘the first volume in the *History of Wales*’, by order of appearance it is in fact the fifth; the first, Kenneth O. Morgan’s survey of the twentieth century, was published as far back as 1981. It is also the first full-scale general history of early medieval Wales since Wendy Davies’s *Wales in the Early Middle Ages* (1982), and at over eight hundred densely printed pages it is easily the longest work ever published on the subject, representing the culmination of a lifetime’s engagement with early medieval Britain and Ireland. Very commendably, Oxford University Press have issued a paperback version at the reasonable price of £30. No obstacle thus stands in the way of this book exercising a profound influence on our perceptions of early medieval Wales – and, as will be seen, many other subjects as well – for years to come.

It is a central message of this volume that the peoples termed Britons in sources of the early medieval period retained a sense that they shared a common origin, a sense that lasted, in however attenuated a form, into the twelfth century. It was, moreover, a perception shared by external observers of the Britons. If this be granted, then it is not only legitimate, but desirable, that Wales before 1064 be studied in a larger context, alongside northern Britain, Cornwall and Brittany. The volume is thus predicated on a phenomenon of the imagination – that is, an identity – which can be recovered only through the painstaking study of written sources. As the author himself acknowledges (p. xix), this is a quite different approach from that of Wendy Davies. Her work was far more informed by archaeology: her book begins with a survey of Welsh geography, climate and settlement, then discusses the workings of the economy and society, and finally reaches the social elites and their imaginative world. In contrast, Charles-Edwards’s emphasis on written sources has produced a book focused on those social elites and their self-projection on parchment and stone. That is a common enough emphasis in writing about early medieval societies. What sets this book apart is the very great interest it shows in all aspects of the production of those written texts on which our histories have to rest. Davies relegated most of her discussion of the sources to a – still very useful – appendix. Charles-Edwards, in contrast, devotes as much space to discussing how words came to be inscribed on these pieces of parchment and stone as he does to exploring what they tell us about the wider society. Questions of authorship and date, and even of the mechanics of early

medieval writing, recur throughout the book. Over and above these, there is a consuming interest in the languages spoken and written in the early medieval Brittonic regions, Brittonic, Latin and Irish. All of this means that the book will be required reading for historians of language and literature as well as those whose interests lie rather in politics and society. It should also provide a convenient place for general historians and archaeologists to go when seeking information on current linguistic and philological debates.

Wales and the Britons is divided into an introduction, which offers a survey of the lands of the Britons from the north of Britain all the way to Brittany, and four large sections. These cover respectively the immediate post-Roman period (c. 400–550, with some extensions); the workings of early medieval Welsh society; the period c. 550–1064 (largely defined by relations with the English); and the church and written culture. No review can do justice to all of the topics touched on in this volume. I will therefore concentrate on what I have already intimated to be the lifeblood of the book, namely what it has to say about textual culture and textual production among the early medieval Britons, and the uses to which the extant sources may be put in the writing of wider history. I apologize in advance for having less to say about political and social history. These have at least been addressed elsewhere,¹ whilst the issues that I have chosen are those most likely to be of immediate interest to readers of *Celtica*.

INTRODUCTION: THE LANDS OF THE BRITONS

Wales and the Britons opens with a survey of the political geography of the territories inhabited by the early medieval Britons. Only occasionally does it read like a topographical account; it has little to say of natural features, and even less of the environmental constraints on human life in those lands. Instead it seeks to plot on a modern map the names of the political entities mentioned in early medieval sources. The approach is similar to the same author's survey of the Irish midlands and north in *Early Christian Ireland*, but, alas, no British equivalent of Tírechán's *Collectanea* survives to guide us through the lands of the early medieval Britons, and so the evidence must be assembled from sources of very disparate age and reliability, and there is not remotely as much of it.

The most difficult and contentious area is northern Britain. No part of this region remained under Brittonic political control after the eleventh century, and most of the British lands had been absorbed into English-speaking polities centuries earlier. This rupture broke the transmission of cultural memory among the northern Britons. Fragments of information can be pieced together from generally reliable external authorities – Roman writers, Bede, the Irish annalists – and a little more from later medieval authors, such as the hagiographers of St Kentigern, who had cause to look back towards the Brittonic past, but there is still an uncomfortable reliance on Welsh poetry

¹For other reviews, see the items by Breeze, Brett, Dark, Higham, Pryce, Roberts and Woolf in the references.

of highly controversial date and equally dubious nature. Uncertainty is greatest in the case of those names, such as *Rheged*, which are attested only in Welsh literary texts preserved in later medieval manuscripts. Charles-Edwards acknowledges the problems with exemplary scrupulousness, yet it is difficult to ignore the possibility that the authors of our extant texts may themselves have been obliged to reconstruct the geography of a lost Old North. As Charles-Edwards himself notes, 'the medieval Welsh could make fundamental mistakes about early northern geography' (p. 10). His example is *Gafran*, which he plausibly identifies with the Cenél nGabráin of Kintyre, but which appears in the Welsh Triads as *Pentir Gafran* in a context which requires it to be taken as the Ord of Caithness. The mistake may be admitted, but it is not clear that the term is used any more 'correctly' in one of the poems attributed to Taliesin, where it occurs in a list of northern places where the honorand, Gwallog, is famous: *ymprydein yn eidin yn adeueawc / yg gafran yn aduan brecheinawc*.² Charles-Edwards suggests that the list encapsulates the different peoples of the north (*Prydyn* for the Picts; *Eidin*, Edinburgh, for the Britons) and so it is likely that *Gafran*, Cenél nGabráin, stands for the Scots of Dál Riata and is therefore being used correctly. This, however, ignores the fourth name in the list, *Brecheinawc*, which is not clearly applicable to any possible fourth group.³ But for the evidence of the Triad, we might be happy enough to accept that *Gafran* does refer to the land of Cenél nGabráin, but the Triad forces us to sit on the fence – unless, that is, we start from the presupposition that the poem is much earlier, and thereby more likely to be accurate, than the Triad. Remove this supposition, and we confront the possibility that medieval Welsh writers inherited a limited range of northern place-names from the British past and used them to create a *Hen Ogledd* of the imagination.

The contrast with the discussion of early Welsh geography is striking: here the sources are still fragmentary and the conclusions just as cautiously worded, and yet we feel ourselves on much firmer ground. The most important point here is that the early kingdoms continued the *civitates* of Roman Wales. Powys is very credibly argued to be a continuation of outlying districts of the Cornovii, the core of their territory having been lost to the English.⁴ Gwent continued the Silures, and Dyfed the Demetae whose name it bears.

²Williams, *Poems of Taliesin*, XI.41–2.

³If correctly identified with Brechin, cf. Williams, *Poems of Taliesin*, 128, then it refers to a region within *Prydyn*, as too would *Gafran* if it is to be taken here, as in the Triad, as Caithness.

⁴I am not convinced, however, by the suggestion (attributed to Marged Haycock, p. 16 n. 77) that *Cernyw* in a poem attributed to Taliesin is a unique survival of their name, rather than the common name for Cornwall. The claim that a Welsh leader aspires to dominate one of the other Brittonic regions is a topos of Welsh praise poetry. Prydydd y Moch boasts that Llywelyn ab Iorwerth will extend his power 'beyond Loch Lomond' and the same poem contains a rather unclear reference to Brittany (Jones, *Gwaith Llywarch ap Llywelyn*, 22.18, 23). In the Taliesin poem *kernyw kyfarchet* could be an aspiration ('let him attack Cornwall'), the termination *-et* being that of the third person singular imperative as well as the impersonal preterite. Note that the next two verbs, *mawl* and *dystwc*, are in the present-future tense and might easily be interpreted as references to the Cornish campaign in the future.

The evidence of the early *Vita Samsonis* is convincingly drawn on to show that these two kingdoms shared a border in the seventh century, as the Roman geography would lead us to expect. Most significant is the insistence that the kingdom of Gwynedd arose in the territory of the Ordovices and represents a continuation (or a replacement) of their polity (pp. 20–1). This point needed to be made, for in recent decades our maps of the tribes of Roman Wales have been determined by Jarrett and Mann's flawed analysis of 1968, with unfortunate consequences.⁵ That article marooned the Ordovices in mid-Wales while leaving the very important region of north-west Wales unclaimed, a vacuum that has encouraged the poorly evidenced Deceangli to creep unjustifiably westwards on our maps.⁶ The upper Severn Valley, where Jarrett and Mann located the heartland of the Ordovices, is a cul-de-sac that opens eastwards towards England. It offers a poor basis for wielding political power within Wales, certainly in comparison with Anglesey and the surrounding coasts. Its medieval rulers, the kings and princes of Powys, might flourish while their neighbours were weak (for instance, in the early years of the twelfth century), but they would struggle to make a mark otherwise.⁷ It is to be hoped that specialists in Roman Wales will ponder the implications of this for their own period.

There is a useful discussion of political fragmentation in early medieval Wales (pp. 18–21). The smaller units which come into view, some of them as a result of fragmentation of the major kingdoms, are identified with the *pagi* familiar from Roman Gaul. It is not quite made clear, however, whether Charles-Edwards sees such entities as Gwerthrynion and Cydweli as continuations of territorial units that existed below the level of the *civitas* during the Roman era, or as simply analogous to them. The truth probably varied from case to case, and in any event evidence for individual *pagi* in Roman Wales is meagre, though Charles-Edwards makes the fair point that the Latin word itself survived into Welsh as *pau*. This is a problem that is not likely to be resolved unless we discover an inscription of Roman date referring to one of the later-attested small territories. There is at least evidence for a *pagus* in Cornwall mentioned in the early *Vita Samsonis*; this is discussed by Charles-Edwards on p. 23.

PART 1: AFTER ROME

The theme of the early chapters is one on which Charles-Edwards has made several important contributions: the process by which the Britons ceased to be a minor subset of the citizens of the Roman empire and came to be regarded by their neighbours as barbarians, a people beyond the pale of cultural and religious normality. Geographical marginalization played a

⁵Jarrett and Mann, 'The tribes of Wales'; see esp. 167–70.

⁶In Salway's authoritative *Roman Britain* (Maps II and V) the Deceangli sprawl over the river Conwy all the way across to the Menai Straits, a development even more unlikely than Jarrett and Mann's original suggestion that they reached the Conwy ('Tribes of Wales', 166).

⁷Cf. the remarks of Davies, *Age of conquest*, 229.

great part in this. Chapter 1, 'Britain, 350–550', provides a political narrative of how the territories of the early medieval Britons came to be defined – meaning, for the most part, reduced. In approximate chronological order, the Britons acquired a dividing line with the Picts through the latter's ethnogenesis in opposition to Roman power; with the Romans, through the collapse of central control over Britain around the year 410; with the English, through the Anglo-Saxon conquests and settlements in eastern Britain; and finally, with the Franks as relations were established on the Breton border. In spite of the expansion into western Armorica, the resulting British territories were not large, nor were they especially fertile, and what was worse, they were strung out along the edges of other, more powerful polities and very vulnerable to further aggression.

The sources for the late fourth and fifth centuries are few and have been very extensively studied. Charles-Edwards steers a moderate course through the debates. Though acknowledging the archaeological evidence for economic disruption in the fifth century, he argues that some kind of political authority survived in Britain after 410, founded on local rulers who were able to draw both on taxation in kind and military service. He is favourable towards Gildas's story of the establishment of Germanic *foederati* in Britain, though he dates it to the early post-Roman years, rejecting Gildas's own highly problematic chronology. He attaches some weight to the evidence of the *Chronicle of 452* and Constantius' *Life of St Germanus*, using them to argue for Anglo-Saxon expansion in Britain before 429 and for a serious episode of conquest c. 441. The narrative is plausible, and attractively tied to the archaeological evidence of burials.⁸ A sensible model of settlement is also proposed, namely that there was an initial conquest by large forces, followed by the arrival of settlers organized in smaller groups. This is much more satisfactory than the scenario put forward in some scholarship whereby these small groups themselves are seen as having been able to disempower the British occupants and take the land, or even settling by consent of the inhabitants.⁹ Though the exact nature of what happened in fifth-century Britain will never be known, it is clear that a military catastrophe was inflicted on the Britons, and that must have required large and well-organized forces.

The section on the origins of Brittany focuses on the evidence for early rulers and their territories, and most particularly on their relations with Frankish kings. Charles-Edwards (p. 73) argues that the settlement began in the mid-fifth century with the agreement of the Roman authorities, and continued into Frankish times through an accommodation reached with the Merovingian kings. This arrangement was only seriously disturbed from the middle of the sixth century, and largely as a result of the political divisions within the Merovingian dynasty, to which the Breton rulers responded with

⁸But see Halsall, *Worlds of Arthur*, 228ff and 276 for a recent rejection of the idea that furnished burials indicate areas of migration from the Continent.

⁹A recent affirmation of this model of very small units, though one that does not see the process of settlement as largely peaceful, is Wickham, *Framing the early Middle Ages*, 313–14. See again Halsall, *Worlds of Arthur*, 270–81 for criticism of the model.

an opportunistic mixture of territorial expansion and submission. This section extracts considerable value from very meagre sources; the cost, however, is to see the Bretons through Frankish eyes, as a factor in Frankish politics. Given the nature of the sources, however, this could not be avoided.

Besides the territorial consolidation of the various small 'Britains', another parting of the ways between the Britons and the other former inhabitants of the Roman empire was linguistic. Latin must have been socially and economically dominant in Roman Britain as elsewhere, and yet the British regions emerged from the post-Roman era speaking dialects of Brittonic, with Latin relegated to the church. This development is the subject of Chapter 2, 'The Britons and their Languages'. To devote a whole chapter to language is unusual in a history book, yet it is wholly justified, for as Charles-Edwards observes, 'what language one spoke had become the principal mark of national identity in Britain as a whole' (p. 75). It is also a topic that can appear impenetrable to non-specialists. Charles-Edwards attempts to overcome this with a potted summary of the development of Brittonic to 700, explaining such concepts as lenition, apocope and loss of declension. These matters are discussed in the wider context of typological drift: some developments in Brittonic (lenition, loss of phonemic vowel length, abandonment of cases) were heavily influenced by processes at work in late Latin, while others (apocope) were shared with Irish and even the Germanic languages. 'British began by behaving as a citizen of the western (and increasingly Latin-speaking) Roman Empire; in the intervening period it partially shared a change with western Romance and with its Celtic neighbour, Irish; it ended by leading a maritime group that straddled the former frontier of Rome.' (p. 87). After this, the separation of the Brittonic languages is discussed, with some attention to Pictish. The old idea that Brittonic began to disintegrate early, following the severing of territorial links between Wales, the north and the west country through the Anglo-Saxon conquests, is treated with due scepticism. Maritime links continued, as is evidenced by the mixture of Welsh, Cornish and Breton features often found within Brittonic glosses in manuscripts. The final major topic of this section is the decline of Latin among the Britons. A strong case is made that Latin continued to be a spoken language among the Britons for some time after the end of Roman rule. The crux here is the widespread confusion of cases visible in Latin inscriptions from post-Roman Britain. The first step in Charles-Edwards's argument is a convincing rejection of Kenneth Jackson's attempts to explain away the confusion through rather convoluted interpretations of the formulae on the stones. Having established that the nominative and the genitive cases are indeed frequently confused in these inscriptions, Charles-Edwards then argues that this is evidence for a spoken, i.e. native, Latin in Britain which underwent developments similar to those on the Continent that produced the Romance languages. One such development was the collapse of declension. It must be borne in mind that throughout this period there was a Christian church active among the Britons which must have used Latin as its liturgical language at least. Were Latin by this time solely a written language, then British churchmen would have had to learn it from

books, and it is hard to see how anyone whose Latin, however elementary, was acquired in this fashion could have perpetrated the ‘errors’ we see in the post-Roman inscriptions of Britain. The same applies, as Charles-Edwards notes, to spellings in the inscriptions which show similarities to sound-changes in Vulgar Latin on the Continent.

The inscriptions in fact form the subject of the dauntingly long chapter 3. This, like the introduction, examines all of the territories of the Britons from Scotland to Brittany. It is well illustrated, the maps being particularly useful, and offers a coherent account of the development of epigraphic script linked to the changing social context of the inscriptions. The inscriptions and other evidence are employed to good effect in Chapter 4, ‘The Britons and the Irish, 350–800’. A major theme of this chapter is the large Irish element among the ruling elite of some post-Roman British regions. The presence of these Irish is revealed by the use of the Irish language, in ogham script, together with the presence of many Irish names in the inscriptions. Charles-Edwards has revised his former scepticism regarding the Irish origin of the name *Gwynedd*, now seeing that kingdom as ‘founded by the Irish, or at least in a very close alliance with them’ (p 178 n. 24; p. 190). That *Gwynedd* is related to the Irish name *Féni* is not in doubt, but it is disputed whether the Welsh term is a straight borrowing of the Irish one or a native cognate. Charles-Edwards now argues that the coincidence in timing between the appearance of *Gwynedd* in the former territory of the Ordovices and the conquests of the *Féni* in Ireland, around the turn of the fifth and sixth centuries, points towards a direct borrowing. The former event may be dated with some confidence through the contrast in phraseology between two inscriptions: the fifth-century Corbalengi stone from Cardiganshire, which describes its dedicatee as an *Ordovs*, a member of the Ordovices, and the Cantiori stone at Penmachno, dated here to the early sixth-century, following Tedeschi, and which is the earliest source to name *Gwynedd*. The dating of the *Uí Néill* conquests at the expense of Leinster in the Irish midlands is a much more complex matter; it is not addressed in detail here, and the reader will have to turn to the same author’s *Early Christian Ireland* for a detailed exposition of his case for a broad date of c. 500, as well as for a discussion of the application of *Féni* in the Irish context.¹⁰ The later Welsh accounts of the founding of *Gwynedd* by Cunedda Wledig are treated with deserved scepticism (pp. 180–1). Since the story of Cunedda is not recorded in any source until after the replacement of his dynasty by that of Merfyn Frych (c. 825), it is tempting to wonder whether the whole account of his migration from Manaw Gododdin to north Wales was in fact a concoction of Merfyn Frych’s time, intended to provide a historical precedent for rule by an intruder. Charles-Edwards does not go this far, pointing to evidence in *Historia Brittonum* (829/30) that the story was already being told in divergent ways in the early years of Merfyn’s reign. The objection is a fair one, and the balance of probability is that the story of

¹⁰See Charles-Edwards, *Early Christian Ireland*, Chapter 11 for the *Uí Néill*, and 160 for the peoples called *Féni*.

Cunedda's southward emigration was indeed already being told in the time of the first dynasty of Gwynedd. That may not, however, be the end of the matter. One of the features of the Cunedda legend most difficult to swallow is its implication that the intrusive founder of the first dynasty of Gwynedd came from a place with the same name as the homeland of the intrusive founder of the second dynasty: Manaw Gododdin in Cunedda's case, and the Isle of Man (Welsh *Ynys Manaw*) in the case of Merfyn. On that basis, indeed, David Thornton questioned the received idea that Merfyn himself came from the Isle of Man, suggesting that the Manx origins of Merfyn were concocted to resonate with the Cunedda story.¹¹ Thornton's arguments are convincingly rebutted by Charles-Edwards in a later chapter (see below), yet that leaves the coincidence in place. A less drastic solution was put forward by Patrick Sims-Williams: that the Cunedda legend was indeed current before Merfyn's time, but that the precise location of his origins in Manaw Gododdin was added to the story under the second dynasty.¹² This is economical and plausible. If it is true, and assuming that the relevant passage belonged to the original text of the *Historia Brittonum*, then the idea must have been of very recent vintage when it was recorded there in 829 or 830.

The remainder of the chapter is largely given over to Christianity and the church. The evidence for what is termed the 'shared ecclesiastical culture of the Britons and the Irish' is lucidly presented, while the terms 'Celtic church' and 'Celtic Christianity' are both condemned as misleading (p. 187). The summariness with which they are dismissed reflects, I suspect, scholarly impatience to leave behind a debate long settled among early Insular historians, but neither term shows much sign of disappearing from more popular presentations of the early Irish and Welsh churches, so perhaps a more extensive rebuttal would have been useful here. The chapter concludes by looking forward to the eighth century, described as an age of 'ethnic consolidation'; the sense of a shared ecclesiastical culture now gives way to a far stronger sense of ethnic division between Irish and Britons, though much peaceful intercourse continued.

Three British authors are the subject of Chapter 5. The writings of Pelagius and Faustus of Riez are arguably more pertinent to the history of the Mediterranean heart of the late Roman West than to that of its north-western periphery, though the association of Pelagianism with Britain was to have a lasting impact on outsiders' views of British and Irish Christianity. Gildas, on the other hand, is fundamental. The section devoted to him here might best be described as a consolidation of advances made in the past decades. The key message is that Gildas's text needs to be understood as a piece of literature in its own intellectual and theological context – and this *before* it is employed in any attempt to write fifth or sixth-century British history. Gildas is not a bungler simply because his account of the fifth century seems to be impossible: he was reconstructing as best he could a past that was already

¹¹Thornton, *Kings, chronologies and genealogies*, 94–5.

¹²Sims-Williams, 'Historical need and literary narrative', 17.

almost lost to knowledge. The *De excidio*, following the work of François Kerlouégan, Michael Winterbottom and Michael Lapidge, is a work of great sophistication and learning, composed in a virtuoso Latin style. Charles-Edwards sees two aspects to Gildas's profounder intellectual achievement. On the one hand, his understanding of the role of the biblical scholar looks forward to that grounding of law in biblical precedent which we see in the *Collectio Canonum Hibernensis*, and suggests that Gildas was an early representative of those ecclesiastical scholars who would be a characteristic feature of the British and Irish churches. On the other hand, Charles-Edwards also accepts Lapidge's insistence that Gildas was educated in the forensic oratory of the late Roman Empire, with the implication that such an education was still available in Britain during Gildas's youth, and hence that there was some need for it: there was 'some form of Roman administration surviving in Britain approximately about 500', but 'the education which sustained it was also put to good use by the Church' (p. 215). That Gildas was himself a major influence on the intellectual development of the British and Irish churches, as previously argued by Richard Sharpe, emerges clearly from this discussion too. The red-herring of the 'northern Gildas' is deftly disposed of on pp. 209–11. The equally unhelpful idea that the text of *De excidio* is not a unity is allowed one dismissive footnote;¹³ Charles-Edwards's fine exposition of Gildas's methods is in any case as decisive a rebuttal of this suggestion as could be desired. On more minor issues: Gildas's Picts did come from overseas; that is the plain meaning of what he says, and he was simply mistaken; and his *Maglocunus* is Maelgwn of Gwynedd, recent (hyper)scepticism notwithstanding. Finally, the traditional date of c. 540 is cautiously affirmed, essentially following the argument of David Dumville: the date of the letter to Aëtius is secure (446 × 454), and the events attributed by Gildas to the succeeding period cumulatively suggest that Gildas conceived of a duration of no less than a generation, followed by the famous forty-four years of relative piece. *Pace* Charles-Edwards's note of caution here, it is not likely that any proposed rearrangement of Gildas's narrative would greatly affect this point: even if the Irish and Pictish raids were to be placed before the letter to Aëtius, the remaining events, those of the Saxon conquests, still give the strong impression of a protracted period of time. The date of Gildas will probably never be known more firmly than sometime in the period c. 510–c. 540, earlier dates being conceivable but unlikely. The general impression I receive from this section is that the study of Gildas is now on a far firmer footing than before the 1980s, though these hard-won gains still need defending.

The final chapter in this section, 'Rome and the Britons, 400–664', largely reprises the author's previous work on the continuity of some aspects of Roman identity among the Britons in the face of an ever-growing feeling among their neighbours that the Britons were un-Roman and even barbarian. The consequences were to be profound, and Charles-Edwards's measured tone should not cause us to forget the sadness of the story he is telling.

¹³203 n. 50.

He offers also a valuable discussion of the material evidence, which draws a distinction between a British elite which held on to Roman cultural markers for longer, and the bulk of the population among whom Roman-style material culture declined much more quickly. There is a familiar problem here in that, over the bulk of Wales, some aspects of Romanization actually become more visible during the post-Roman era than ever before – principally through the evidence of the inscriptions. Our understanding of the post-Roman elite in Wales is hampered by lack of clarity as to their role before then. The conventional division of Roman Britain into a civilian zone and a military zone is, it should be remembered, a convenient if rough classification of the archaeological evidence. It does not correspond to any known administrative division, and we do not know what these zones would have been called in Latin, if they were conceptualized at all. Regrettably, the habit has established itself within Romano-British studies of using the Latin term *civitas* to refer only to those to self-governing units which have archaeologically visible urban centres, and thereby denying that the Roman state recognized any *civitates* in the townless ‘military zone’; the latter would have remained under ‘military administration’, a concept whose practical reality needs far more discussion than it has received. To a Roman, however, a *civitas* was simply any polity, a category that certainly included the tribal units of Gaul and Britain both before and after they were conquered. Charles-Edwards is aware of the problem (cf. p. 17, n. 87), but he slips into the usage here (p. 221) nonetheless. The point is significant because it implicitly denies the Ordovices a role in the Roman system such as the Silures and Demetae can be seen to have had. Though our knowledge of late-Roman military installations is incomplete, it is clear that north Wales after the second century was not a land under occupation; the number of soldiers stationed there must have been modest, and at least part of their role was defending the coast against the Irish. We should see the Ordovices, too, as integrated into Roman system. Local administration and the gathering of taxes must have been largely in the hands of the Ordovician elite, as elsewhere in the Roman world, and the facts that the Ordovices had no town (or none that we have found so far) and played host to a few soldiers do not change that. The importance of Roman cultural markers among the post-Roman elite is hard to account for otherwise, unless we evoke an influx of refugees from eastern Britain.¹⁴ Incidentally, the chapter also contains a new and convincing explanation of one of the most famous of those markers, the ‘consular’ inscription at Penmachno, and an important statement on the date of the early *Vita Samsonis*.

PART 2: EARLY WELSH SOCIETY

I will deal far more briefly with this section, since I feel poorly qualified to evaluate it. Four chapters are brought together here: 7, ‘Charters and Laws’,

¹⁴The problem of how to conceptualize the so-called military zones of Roman Britain affects how we understand the Roman/post-Roman transition in the north too, including the area beyond Hadrian’s Wall.

is source-criticism and prepares the way for the other three, on 'Lords, food-renders, and peasants', 'Kinship and status' and 'Kingship'. The Llandaf charters, and those attached to Lifris's *Vita Cadoci*, are fundamental. Much of Chapter 7 is given over to a comparison of instances where the same grant is recorded in both the Llandaf and the Llancarfan collections. Here Charles-Edwards shows that the former cannot be derived from the latter, but that both versions must reflect an earlier record. This case for the antiquity of elements within the Llandaf charters adds weight to the previous arguments of Wendy Davies and Patrick Sims-Williams based on the witness lists. The discussion of the laws is likely to be more controversial, since Charles-Edwards argues in favour of a major role for Hywel Dda in the formation of the Welsh legal corpus. Chapter 8 offers a highly technical discussion of the terminology of landholding and food renders, and an exploration of the model of the 'multiple estate'. Chapter 9 distinguishes importantly between the kinship of inheritance and the kinship of marriage and alliance. It is enriched by drawing on literary texts as well as the charters and the laws. There is a difficult, and not entirely conclusive, discussion of the term *hereditarius* used in the Book of Llandaf.

Chapter 10, on kingship, has to deal with the difficult issue of the origins of Welsh kingship: was it freshly created out of the ruins of the Roman order, or did it have deeper roots? Both possibilities are discussed, but significantly the section on 'the native element' is several times as long as the treatment of the Roman inheritance. As was noted earlier, our imperfect understanding of how the elites of Roman Wales were integrated into the Roman order is a particular problem here. Another is the extreme fluidity of medieval Welsh terms for rulers. Several are discussed in depth. An interesting suggestion is that *gwlad*, like its Irish cognate *flaith*, could refer to a ruler as well as a polity. This certainly warrants a more detailed study. Yet another problem is the persistence within medieval Welsh mythmaking of the idea of the king of all Britain. Charles-Edwards intriguingly suggests that the idea had its origins in the Lucius legend, which implied that a British king ruled under the overlordship of the Roman emperor (p. 322). The discussion of royal succession, which is very reminiscent of Charles-Edwards's treatment of its Irish counterpart,¹⁵ argues for the importance of the kinship of marriage in deciding which of several qualified candidates might succeed; the argument contextualizes the well-known genealogical section of *Historia Gruffudd vab Kenan*, for instance. A curious feature of this chapter is that it now returns (p. 334) to the nagging question of Roman and native elements in kingship, as if aware that no definitive answer had been given, and yet it is hard to feel that the ensuing discussion of Gildas's kings really moves us much further forward. It is generally accepted that they represent kingships of no great antiquity, and yet, in Charles-Edwards's view at least, the terminology of Welsh kingship had its roots deep in the Celtic past. How, if so, did it survive

¹⁵ *Early Christian Ireland*, 92–6.

the Roman centuries? The unRomanness of medieval Welsh law, as revealed in the thirteenth-century law tracts, poses a similar question.

PART 3: THE BRITONS AND THE ENGLISH, 550–1064

At seven chapters, this is by far the longest section of the book. Much of it is given over to detailed narrative. As the title suggests, the treatment consistently regards medieval Welsh rulers as operating in the shadow of far more powerful neighbours, a view with which it is difficult to disagree. I will largely leave the narrative sections alone, but there are some important readings of key texts here which deserve some comment.

The much debated lost northern British source thought to lie behind parts of the *Annales Cambriae* and the *Historia Brittonum* is the subject of a lengthy discussion. No final conclusion is available regarding its nature, but Charles-Edwards does put forward a case that it was independent of the recording of British events in the Irish annals – so that the Irish and the Welsh texts may be compared as independent sources for events. The most controversial source, however, is likely to be the *Gododdin*. Charles-Edwards suggests that the identification of Rhaeadr Derwennydd in *Pais Dinogad* with Derwentwater in the Lake District, first put forward by R. Geraint Gruffydd,¹⁶ is correct and presupposes an addition to the text made, in manuscript and not orally, in that region; since the supposed British occupant of that territory, the kingdom of Rheged, perished in the seventh century, that takes the manuscript text of the *Gododdin* back to that century. Furthermore, since *Pais Dinogad* is not in the B-version of the *Gododdin*, the shared archetype of the two versions is even older, though not earlier than 642, the earliest possible date for the Strathcarron stanza which is common to both. That stanza itself indicates a period when the shared archetype was preserved in the British kingdom of Dumbarton. It should be admitted that there does not seem to be anything impossible in this reconstruction. Yet the identification of Derwennydd with Derwentwater is bold, and even if it is accurate, it is even more bold to assume that the poem can only have been added to the text in that locality; it could have been preserved separately and only added to the text of the *Gododdin*, with which it appears to have no relationship, in Wales and at any date subsequent to the division of the transmission into A and B strands. It should be noted, in any event, that the B-text is physically incomplete in the manuscript, and therefore any arguments based on the absence of elements from it are hazardous. The Strathcarron stanza too need not have been added in Dumbarton. Thus the crucial early stages of this reconstruction rest on some courageous assumptions. This is a problem, for the early date of the archetype is crucial to the second part of Charles-Edwards's argument, namely that the very wide textual variation between A and B can be turned to the historian's advantage. He suggests that it is possible for a historian to use words which

¹⁶Gruffydd, 'Where was *Rhaeadr Derwennydd*'.

occur in (roughly) the same position in the A-text and the B-text; after those, it is possible also to use words found in the B-text, though not in A, provided that the stanza itself is also in A (and thus was in the archetype of A and B). If the archetype, however, is not as old as the sixth century, then it is unclear how far back such an approach will take us.

This is an important attempt to rescue the *Gododdin* from the complete historical oblivion to which David Dumville consigned it, and it is likely to incite fresh attempts to use this text to write sixth-century history. The possibility that literature in British survived from the northern realms, even from a very early date, and passed into Wales, should be admitted. Indeed, it is very likely that this did happen and that it provided a route of transmission for some of the northern personal and place-names recorded in later Welsh literature. That *some* thread of transmission endured from sixth- or seventh-century northern Britain is clear from the survival of the name *Gododdin* itself, which is a regular Welsh reflex of a tribal name attested in Ptolemy. The problem arises from the belief that what we have in our thirteenth- and fourteenth-century manuscripts has any relation to actual texts of the sixth century, as opposed to representing the terminal stages in a tradition of telling stories about the Old North and of creating poems voiced in character that reflected those stories. And finally, even if the ‘authenticity’ of the *Gododdin* as a product of the later sixth or early seventh century were to be admitted, the immense differences between versions of the ‘same’ stanza in the A-text and the B-text make it hard to see how any single line or phrase can be used safely as historical evidence.

Chapter 12 opens the narrative history of the period, dealing with events to 685. It is very clear and well-told. True to the opinions expressed in the previous chapter, the narrative incorporates material from the *Historia Brittonum*, *Annales Cambriae* and the *Gododdin*, and will therefore be controversial in parts. There is an important and welcome rejection (p. 390) of Alex Woolf’s suggestion that we should not identify Bede’s Caedwalla with Cadwallon of Gwynedd, but rather with a putative northern figure (for whom there is more or less no evidence). Since the suggestion has been repeated by others, I should have liked to see a lengthier rebuttal here. The logic of the situation points clearly to the correctness of the traditional interpretation. Edwin of Northumbria had just conquered Anglesey, the heartland of Gwynedd: a response by Gwynedd was essential if the kingdom were to survive in any meaningful form. That Bede describes Cadwallon’s actions as revolt would be perfectly reasonable from his point of view, for the conquest of Anglesey implies that its ruler had submitted to Edwin. The most interesting part of the chapter, however, is a detailed discussion of the fate of British Christians within Northumbria. The importance of the Easter problem in the marginalization of British cultural identity is clearly brought out, with which we may compare the account in Chapter 4 of the exclusion of the Britons from Romanness. The narrative continues in Chapter 13, which covers the period of Mercian supremacy, 685–825. This includes a discussion of the fates of Dumnonia and Cornwall, and of the Britons of Dumbarton.

We return to source-criticism in Chapter 14, ‘Two Ninth-Century Writers’, which discusses the anonymous *Historia Brittonum* and Asser’s Life of King Alfred. It is an excellent and sensitive discussion of the *Historia* as a literary as well as a historical text; in the past the *Historia* has sometimes been approached simply as the latter, so a discussion like this is particularly necessary (as is the case with Gildas, see above). We also find a welcome rebuttal of two particularly unhelpful ideas in the current scholarship. One is that the various churches bearing the name of St Garmon were originally dedicated to a local saint, who only subsequently became identified with St Germanus of Auxerre. This is an idea for which there is neither any evidence nor any need, and its continuing prevalence seems to be the result of the lingering influence of the idea that Welsh churches were invariably founded by the individuals whose names they bear, coupled with some minor linguistic concerns regarding the form *Garmon*. The consuming importance of St Germanus of Auxerre in the *Historia Brittonum* – as indeed in other Insular texts – should render it easy to accept that churches were founded in his honour in early medieval Wales; they do not, it should go without saying, have to be as old as 429. The second unhelpful idea is that *Historia Brittonum*, being a composition made in Gwynedd, must of necessity be a propagandistic work written to favour the Gwynedd dynasty. Nothing in the structure or content of the work offers any support for this misplaced endeavour to force it into the paradigm of ‘historical need’. The question is most important in regard to the origin story of the dynasty of Cadell of Powys, §§32–5 of *Historia Brittonum*. Cadell here is a slave raised to the kingship by St Germanus. Charles-Edwards rightly gives short shrift to the notion that this is a piece of Venedotian propaganda designed to blacken the reputation of the Cadelling, pointing to the biblical parallel of King David, who was raised from a shepherd boy to royalty. The story, rooted in the geography of north-eastern Powys, should come from a religious centre in that area, probably Llanarmon-yn-Iâl. It might be added that some at least of the material assembled on the dynasty of Gwynedd was far from flattering to that dynasty – the story of Cadafael the ‘battle-shirker’, for instance (§65). *Historia Brittonum* is, as numerous studies by David Dumville have insisted,¹⁷ a sophisticated work of historical scholarship struggling to tell a coherent story using a meagre body of unhelpful source material. As propaganda for Gwynedd, it is singularly ill-focused.

The remaining chapters in the section are almost wholly narrative. They cover the ninth century; the period of the ‘empire of Britain’, that is, the first half of the tenth century; and finally the period 950–1064, including a section on the last century of the kingdom of Cumbria/Strathclyde. Charles-Edwards ably defends the traditional view that Merfyn Frych, founder of the second dynasty of Gwynedd, or the Merfynion as they are called here, came from the Isle of Man, and offers a plausible account of the circumstances that allowed Merfyn to take power in Wales. He also provides a reconstruction of the stages by which that dynasty’s power extended first over Powys and

¹⁷See, e.g., ‘The historical value of the *Historia Brittonum*’.

then into south-west Wales. The period from the late eighth century, at which point the Welsh annals become a contemporary record, is the first in Welsh history for which a detailed political and dynastic history can be written. The difference in what we can say about Wales before and after *c.* 800 is very noticeable here; it is now possible to follow the transmission of power in Wales between individuals and generations. The framework is still, however, that of English politics, leavened now with references to the Vikings of Dublin and the Isles. This lends poignancy to the indisputably Welsh voice of the author of *Armes Prydein*. The controversial question of the date of this poem is discussed on pp. 519–35. As is common with prophetic texts, *Armes Prydein* is difficult to date satisfactorily because of the vagueness of its allusions and the lurking danger of taking topoi too literally. The poem's allusions to Brittany joining in an anti-English coalition must be an example of such a topos, as David Dumville has shown, and can have no bearing on its context.¹⁸ Debate on the exact date within the broad mid-tenth-century period is likely to continue, but one point may perhaps be dealt with. The line *Gwyddyl Iwerdon Mon a Phrydyn* has proved controversial.¹⁹ Commentators have taken *Mon* to be dependent on *Gwyddyl*, as is *Iwerdon*. There have been several attempts to explain this, two of which are convincingly rebuffed by Charles-Edwards here. One is to take *Mon* as the Isle of Man; the other, by Colmán Etchingham, to redate the poem to the eleventh century, when Irish kings had supremacy over Dublin and its dependencies, perhaps including Anglesey. Charles-Edwards suggests (pp. 528–9) rather that there was a Gaelic-speaking colonization of parts of Anglesey following the expulsion of the Dublin Vikings in 902. However, perhaps the simplest way of resolving the problem is to take *Mon* independently of *Gwyddyl Iwerdon*; the reference is not to Goidels of Anglesey, but Welsh of Anglesey (and, by implication, the rest of Gwynedd). Certainly there is nothing in the Welsh to require us to take either *Mon* or *Prydyn* as dependent on *Gwyddyl*. If it be objected that *Gwyddyl Iwerdon* is a tautology, we might recall that this is poetry, and the poet was seeking after a certain line-length and a satisfactory internal rhyme. *Prydyn*, finally, is simply the kingdom of the Scots. That Anglesey (and Gwynedd as a whole) should already be comprehended within the word *Kymry* in the previous line is also not a major objection given the strong focus in the poem on south Wales. What we have here is a summary of the coalition desired by the author: it includes Gwynedd alongside the Norse of Dublin, the Irish of Ireland, the kingdom of Alba, Cornwall and Strathclyde. This removes any necessity to worry about settlements in Anglesey, be they by Norse-speakers or speakers of Gaelic.

PART 4: THE WELSH CHURCH AND CULTURE

The final three chapters return to thematic surveys. Of these, the most substantial is Chapter 18, 'The organization of the church'. Every aspect of early

¹⁸Dumville, 'Brittany and "Armes Prydein Vawr"'.
¹⁹Williams, *Armes Prydein*, 10.

medieval Welsh bishops is problematic – their number, their location and their spheres of jurisdiction – and the problem is one of inadequate sources at every stage. The sources are inadequate because there are not enough of them and because their laconic references to episcopal activity and jurisdiction presuppose an understanding of these matters among their readers which is precisely the thing which we now lack. The starting point, as Charles-Edwards explains, should have been the system recorded elsewhere in the Roman world, whereby each *civitas* or urban centre should have had a bishop whose jurisdiction matched that of the dependent territory of the *civitas* in question. Sadly, bishops are not attested at Carmarthen, the urban centre of the Demetae, nor at that of the Silures in Caerwent. That these two places were the seats of bishops at the end of the Roman era is thus a guess, albeit a plausible one. Central and northern Wales pose the even more intractable problem that they had no urban centres, or at least none that we know of, nor do we understand at all well the politics of this area, as the reader will by now be aware. If, therefore, the Ordovices had a bishop in the period before they vanish from the record, the base of his operations is unknown.

To put it bluntly: the earliest postulated stage in the organization of the Welsh church is unrecorded. Regrettably, what sort of arrangement succeeded this putative early stage is if anything even less clear, for though during the following centuries the evidence for bishops in Wales does start to accumulate, we have no agreed model into which to fit it. Indeed, it must be doubted whether the very sparse and scattered Welsh evidence offers enough on its own to support any model. It is impossible to resist the temptation to dip into the much richer sources for England and Ireland, but that leads inevitably into the quagmire of early Irish church organization. Here there is at least no lack of sources, but there is the same inconvenient assumption on the part of their authors that their readers required no explanation of the physical extent of bishops' authority. Much debate has focused on the meaning of the Latin term *parochia* (or *paruchia*), to which Charles-Edwards devotes several lucid pages (pp. 583–6). It emerges that the word could refer both to the territory subject to a particular church and to a network of dependent churches and lands. The term means something like 'sphere of authority attached to a major church', and as such its meaning was as variable and flexible as the definition of that sphere and the kind of authority under consideration. On its own, the term will not allow us to determine whether a bishop's authority was exercised within a single coherent block of territory or over a series of such blocks attached to dependent churches and scattered across the country, nor will it elucidate the relative importance of economic lordship and pastoral care, another major aspect of the historiographical debate which Charles-Edwards largely bypasses here.

Nevertheless, the possibility that a bishop's *parochia* could be geographically discontinuous opens the question of what kind of arrangement succeeded the clearly defined and territorially contiguous dioceses of the late Roman period. Charles-Edwards assembles several pieces of evidence that the number of bishops increased, and that there could be (and was) more than

one bishop in a kingdom. Some of the evidence presented is not strong: in spite of Charles-Edwards's reasoning, *sacerdos* in Aldhelm's letter to Geraint of Dumnonia is too ambiguous a word on which to hang an argument, since it can refer both to bishops and priests indifferently, and a later Welsh document cited on p. 591 shows it being used specifically in contrast to a bishop. Nevertheless Bede's reference to *episcopi* within a single British *provincia* is suggestive. Beyond this lies the question of how many such bishops the evidence will accommodate. The thrust of Charles-Edwards's arguments is that there may have been many. For instance, the tract on the seven 'bishop-houses' of Dyfed, preserved in the Welsh Laws, might suggest that as many as seven bishops operated from churches within that small area, though Charles-Edwards acknowledges that the 'episcopal' nature of these churches may have been historical, that is to say, that not all of them ever possessed a bishop at the same time. There is certainly good evidence for early medieval bishops at Llandeilo Fawr and Glasbury, and more dubiously at Dewstow in Gwent, yet it remains difficult to imagine seven individuals of episcopal status operating within the confines of a territory as small as Dyfed.

More broadly, Charles-Edwards accepts the existence of networks of churches dependent on the greater churches and bearing the names and dedications of the major saints – David, Teilo, Cadog, Cynidr etc. The model of the dispersed monastic *paruchia* has been heavily dissected in recent scholarship, but it may be argued that it is the specifically monastic part of the model, rather than the dispersed *paruchia* per se, which has been discredited. The networks themselves are hard to argue away, given the abundant evidence for those of Armagh/Patrick and Iona/Columba, and the tolerable evidence for others such as that of Clonard/Finnian. An understanding that such networks did not consist of monasteries isolated from the lay community around them, but rather of churches with a mixed pastoral and ascetic mission which discharged pastoral duties over dependent territories, allows us to begin to see how non-contiguous dioceses might have worked in practice. A serious difficulty, however, remains: how to be certain that such networks of dependencies were subject to the episcopal authority of the centre, rather than mere economic lordship. Did, for instance, the chief bishop of the David network, presumably based at St Davids for much of the time, exercise episcopal authority and pastoral care over David churches in Gwent, or did he only have the right to take renders from them? The debate looks set to continue.

Moving from bishops to churches, Charles-Edwards offers a description of how a *clas* or mother church functioned, depending heavily on the rich evidence of Lifris's *Vita Cadoci*. There is some hesitation as to whether these institutions should be compared with Anglo-Saxon minsters (pp. 611–12); the number of such sites in the small area of Eryng suggests to Charles-Edwards that Wales may have had a greater density of churches served by a community than England, but the matter is very uncertain. The chapter concludes with a general discussion of the cult of saints in early medieval Wales. The connection between sanctity and cultural identity is explored through the genealogical text known as *Bonedd y Saint*. The idea of different

constituencies for saints' cults is discussed, albeit in a much briefer fashion than the author's work on Irish cults.²⁰ There is an extended treatment of the cult of St David, in which it is argued that the saint's prominence in *Armes Prydein* implies that his pre-eminence was already accepted, perhaps even in north Wales. Many of the comparanda in this chapter are drawn from Irish evidence, and in conclusion Charles-Edwards justifies this by pointing to the close links between the Welsh and Irish churches; communication with the English church, in contrast, was greatly hampered from the mid-seventh century by the bitter disagreement over Easter.

The rather shorter Chapter 19 follows the story of Latin learning in Wales. Having argued earlier that Latin remained a native spoken language, that is, one passed down from parent to child, in Wales into the sub-Roman era, Charles-Edwards is constrained now to establish when it ceased to be so. The evidence of inscriptions is inconclusive since it is hard to distinguish traits inherited from late spoken Latin from grammatical errors perpetrated by people who had learned their Latin in school, but badly. It is also important to note that Latin learned in this way was still a spoken language, certainly within church schools and perhaps more widely among churchmen; evidence for this is offered by such texts as the colloquy known as *De raris fabulis* (treated on pp. 647–8). Charles-Edwards determinedly reads the rather exiguous Welsh evidence for ecclesiastical scholarship in the light of the better-studied Irish material. He concludes, reasonably, that there is evidence that major Welsh churches employed professional scholars analogous to the figure known as the *scriba* in early Irish sources; this was part of a wider shared intellectual and religious culture, as argued passim throughout the book.

The final chapter, 20 ('Poets and storytellers'), moves to the vernacular literature of early medieval Wales. It is a rather eclectic survey, partly because important texts such as the *Gododdin* and *Armes Prydein* have already received detailed treatments. A central, though characteristically understated, message of this final section is that scholars of early Welsh literature would do well to pay attention to the revolution that has swept through early Irish literary studies in recent decades, and appreciate that there must have been the most intimate relations between the carriers and creators of vernacular literature and the institutions of the church. The point is anticipated by Sims-Williams's very convincing argument that the Fourth Branch of the Mabinogi was shaped by the concerns of Clynnog Fawr, acknowledged here (p. 655), though there appears to be a reservation expressed later (p. 668).²¹ Another important argument is that the poverty of early vernacular manuscripts from Wales is an illusion of survival rather than a reflection of fact: Welsh manuscripts survived in the institutional libraries of England, not back home in Wales, and those libraries had use only for Latin manuscripts, not any in Welsh. The argument is a solid one, even if we do not accept the author's view that there is evidence for manuscript transmission of the *Gododdin* as far back

²⁰'Early Irish saints' cults and their constituencies'.

²¹Sims-Williams, 'Clas Beuno and the Four Branches of the Mabinogi'.

as the seventh century (see above). The chapter contains detailed treatments of two poems traditionally attributed to the Old Welsh period, *Edmyg Dinbych* and *Echrys Ynys*.

The dating of *Edmyg Dinbych* and *Echrys Ynys*, which relies on historical arguments, has so far held up against scrutiny. Other datings are more vexed. Ever careful to give fair representation to diverse views, Charles-Edwards acknowledges that the traditional dates of much possibly pre-Norman Welsh literature are open to doubt (pp. 652–5). For instance, Marged Haycock's proposed redating of much of the contents of the Book of Taliesin to c. 1200 is acknowledged. He notes, too, the important shift that 'differences that were once thought to be chronological are now more likely to be ascribed to genre' (p. 653). It is a strong possibility, however, that this shift has further to go than the position defended by Charles-Edwards. The saga *englynion*, whose dating to the ninth and tenth centuries is accepted in this book, are a case in point. Reliance on Jenny Rowland's dating (p. 653) merely underlines the need for a new examination of this question. Is a date in the twelfth century out of the question for the poetry of Llywarch Hen and Heledd? The mere fact that verse in a similar genre and metre is attested in the early tenth century Juvencus *englynion* is not of itself enough to show that those *englynion* which are preserved in fourteenth- and fifteenth-century manuscripts are as old as the Juvencus stanzas. Nor should the twelve poems selected by Ifor Williams as the genuine work of the sixth-century Taliesin be regarded as immune from scrutiny. At the very least, they too need to be reinvestigated in the light of Haycock's work on the rest of the manuscript.

Wales and the Britons draws to a close with a leisurely consideration of some of the vernacular texts, poetic and prose. There is a sensitive reading of some of the Llywarch Hen *englynion*, in a practical criticism mould, and then a final discussion of the role of the storyteller and the poet within early Welsh society. Once again, the comparanda are Irish. It is a fitting conclusion to the volume as a whole that its last words should be a careful, in fact noncommittal, weighing of the relative importance of the 'shared Celtic inheritance' on the one hand, and 'cultural interchange across the [Irish] sea' on the other.

FINAL THOUGHTS

Wales and the Britons is a vast book. It treats of a wealth of topics that one would not necessarily expect to find under that title. There is, for instance, so much detailed engagement with Anglo-Saxon history that the book demands to be read by historians of Anglo-Saxon England. Likewise, the sections on Brittany are important for early Frankish history. It is to be hoped that the relevant sections will be discovered and read by those who need to do so, for there is a risk that this may not happen. For this reason, indeed, I wish to draw attention here to one of the minor stars of the book – the Isle of Man. If the Gaelic language of the Isle of Man is a 'cinderella' of Celtic studies,²²

²²Thomson, 'The study of Manx Gaelic', 177.

then the Brittonic element in the island's history is even more badly served by scholarship. Running through *Wales and the Britons* is a thread of argument that demands the attention of anyone concerned with the history of Man before the Vikings. Out of the, admittedly poor and exiguous, sources, a case is constructed for an important, indeed probably dominant, Brittonic element in the island's culture and politics before the Viking impact in the tenth century tipped the balance firmly towards the influence of the Gaelic world. So complete was that shift that almost all evidence for Brittonic language has vanished from the island's onomastics. Yet, cumulatively, the fragments of evidence assembled by Charles-Edwards here suggest that Man before c. 900 was one of the lands of the Britons; naturally it was in contact with the Gaelic world, but then so were the other Brittonic regions.

Wales and the Britons recalls the same author's *Early Christian Ireland* in that it presents a series of studies rather than an overview of the whole subject. Each individual section engages in intense scrutiny of a single source or of a particular crux. To some degree the approach was pre-ordained by the sources themselves, for during the period covered by this book all of the Brittonic areas suffer from very poor documentation, and a history written from scattered, fragmentary and recalcitrant materials cannot help but be incomplete, nor can it shirk the need for technical discussion. It does, however, make for very demanding reading. *Wales and the Britons* will not, and should not, replace Wendy Davies's *Wales in the Early Middle Ages* as the basic introduction to its subject. Instead, readers will go to this book to consult individual sections as required. They will need to do so, for the author's careful presentation of conflicting arguments ensures that each section serves as a *status quaestionis* and bibliographical reference point for the subject it discusses. Yet for all his judiciousness, Charles-Edwards is never one to sit on the fence. Every single discussion in his book concludes with a judgement on the question under debate. Historians of many and varied interests will be arguing with him for decades to come.

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